



## Constraints on Construction

Yves Gingras; Silvan S. Schweber

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## Constraints on Construction

**Yves Gingras and Silvan S. Schweber**

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Andrew Pickering, *Constructing Quarks: A Sociological History of Particle Physics* (Edinburgh: Edinburgh University Press; Chicago: University of Chicago Press, 1984), xii + 418 pp., £20.00/\$30.00. ISBN 0-85224-458-4.

The advances of the past thirty years in our understanding of the composition of matter at the sub-nuclear level have constituted an intellectual achievement certainly as great as, if not greater than, the unravelling of atomic structure brought about by quantum mechanics in the mid-twenties. Physicists have formulated a renormalizable theory of the strong, electromagnetic and weak interactions that works and that seems to account for all the data for energies below 1TeV. It is a Yang-Mills gauge theory, based on colour SU(3) and electroweak SU(2)  $\times$  U(1) with 3 families of spin  $\frac{1}{2}$  leptons and quarks, their antiparticles and some Higgs bosons.

If the meanings of these terms are not clear, reading Andrew Pickering's *Constructing Quarks: A Sociological History of Particle Physics* will make them so. His book provides a lucid introduction to, and a thorough explanation of, the concepts of modern particle physics. But it is much more than that: it is an important, interesting, admirably documented, detailed history of the construction of the 'standard model' presently deployed in high-energy physics [HEP], written in a clear, unpretentious style piqued here and there with wry humour. Only someone with Pickering's technical background — a PhD in theoretical high-energy physics — could have carried out the task he undertook. The historical, sociological and physics components of Pickering's presentation will certainly impress the scientific community — but, as importantly, the book should also be accessible to the interested general reader who is equipped with a 'basic scientific education' and is willing to expend the effort to understand popular scientific writing at the level of the articles to be found in *Scientific American*. Pickering's exposition is rich in insights and reflects his command of the subject matter. Indicative of this mastery is the dearth of equations to be found in the book. Explanations are never verbal translations of equations. Rather, they depend on analogies and the development of intuitions based on readily visualizable examples. This impressive ability to expound arcane mathematical and physical concepts is but one of the book's achievements. Pickering has also succeeded in making clear the important progress that quarks, gauge theories, quantum chromodynamics (QCD), and grand unified theories (GUTs) represent. In his exposition, he has rightly emphasized how the interconnections among concepts give a physical theory its lasting structural beauty

and its stability. Pickering's account will undoubtedly be amplified and refined in the future, but there is no question that he has given an accurate, impressive presentation of the history of HEP from the end of World War II to the beginning of the 1980s, an account that is erudite and sensitive and that will be the point of departure for future investigations. As his footnotes indicate, Pickering carried out a great deal of original historical researches in the writing of his book. There is much that is new in it, for example, recollections by many of the leading participants, sociological facts about the community, and the like. But Pickering's singular achievement is that he has given not only a perspicuous exposition of the intellectual advances and their genesis but that he has also indicated how these relate to the practices of the community.

Pickering's exposition has the great merit that the experimental and theoretical aspects of HEP are intimately interwoven: their intertwined, symbiotic relationship is in fact an essential aspect of the thesis which structures his history. The history of high-energy physics from the fifties on (HEP as a separate field did not exist before then) is the history of a partnership between theoretical physicists and their experimental colleagues who built ever bigger machines. The experimental findings were the grist for the theoreticians' mill, and the latter's theories and hypotheses the stimulus for the building of ever more energetic machines.

Pickering's history begins with a presentation of the developments in high-energy physics from 1945 to the mid-1960s. The great theoretical advances of that period — renormalization, parity non-conservation, V-A theory, SU(3), dispersion theory, Regge theory — are lucidly explained. The relation of the theoretical practice to that of the high-energy machines of the 50s and early 60s is carefully elaborated. The character of the 'old physics', the dominant practice until the mid-60s, in which the most common and abundant processes involving the hadrons at relatively low energies are investigated and analyzed, is carefully delineated. The changes brought about by the experiments on proton structure at the Stanford Linear Accelerator in the late 60s — the parton model, scaling, colliders, and so on — are characterized by Pickering as forming a new research tradition, the 'new physics', in which relatively rare hadronic processes or equally rare leptonic process (involving only electromagnetic and weak interactions) are investigated. *Constructing Quarks* is Pickering's account of why and how the 'new' physics replaced the 'old'.

As far as theory is concerned, the 'new' physics generalizes quantum electrodynamics (QED). Although QED was invented by Dirac in 1928 shortly after the advent of quantum mechanics, it was only after World War II that it was shown that the divergence difficulties which beset all higher order calculations in QED could be circumvented by a process known as 'renormalization'. Renormalized QED allows finite, definite predictions to be made to every order of perturbation theory. The 'standard model' elaborated during the 1970s generalized QED to include the strong and the weak interactions along with electromagnetism, quarks and neutrinos along with the electrons, and the (still mysterious) other leptons — the muons and the taus — along with the electrons. Pickering's account illuminates how these developments took place. His book will be very useful to the historian, philosopher and sociologist of science and we believe that a great deal of it will also prove quite revealing to physicists, particularly high-energy physicists. His presentation sheds light on the role played by the outstanding contributors as initiators of research traditions, on the role of the theoretician's tool kit in structuring strategies to exploit opportunities that present themselves, and much

else. Occasionally, there will be disagreement with Pickering's assessment of historical events (for example, his account of the impact of the existence of the third family of spin  $\frac{1}{2}$  leptons), and others might put a different stress on certain theoretical questions (for example, the role of gravity), but the coherence and cogency of Pickering's account is truly impressive.

The 'new physics' experiments concentrate on the exploration of rare phenomena, the 'gold plated' events. This requires different kinds of accelerators, different kinds of detectors, different experimental techniques, different electronics, a greater reliance on on-line computing (because the size of the data became much larger), a deeper understanding of statistics, of modelling, as well as a more complete understanding of the physics of background events. All this is succinctly detailed in Pickering's book. Here again, further elaborations and clarifications will undoubtedly be made in the future by historians and philosophers. For example, Pickering does not emphasize the differences in the time-scale of experiments — it usually takes between one and three years to design, tool up and actually run an experiment — and those of theory formation. The constraints imposed by the time-scales of actual experiments on the dynamics of the interaction between theory and experiment are not discussed. Similarly, the time-scale for building new machines affects the kind of theorizing that is done in the slack period when the data of the old machines have been exhausted and the new machines have not come on line yet. But these are minor quibbles.

All this said, we shall now comment in more detail on the implications of Pickering's 'relativism' for the structuring of his argument and the selection of some of his examples. Our criticisms relate principally to the philosophic assertions advanced by Pickering in Chapter 14 and at several other places in the preceding thirteen chapters, and do not affect the main body of his account which, as we shall see, does not depart significantly from the supposedly more traditional 'scientists' accounts', contrary to what the author frequently suggests. In fact, his chronology and periodization differ so little from that spontaneously generated by the HEP community that his book will no doubt be well received by the physicists, who will pay little attention to the peripheral philosophical concerns of the last chapter.

### **Sociological History, Historical Sociology and Relativism**

A historian's work is always based on a particular philosophy of history. So-called 'neutral' accounts are no exceptions, for they too are theory-laden in the very choice of the events they choose to recall. The difference between historical accounts does not lie in the fact that some are more 'objective', while others are less so because of their theoretical presuppositions. Rather, the principal difference is between historians who are conscious of the model at work in their narrative and who present it at the beginning of their work, and those historians who believe that they are free of any 'a priori' model, and hence leave the reader the task of discovering the implicit philosophical foundation of their work, by analyzing the structure of the narrative. Very often the reader will find that the latter historians take for granted actions and beliefs that should be seen as problematic and in need of explanation. Historians of science (and historians of physics in particular) often tacitly share the scientist's assumption that it is 'self-evident' that persons should be excited by discoveries, intensely interested in the detailed working of nature and committed to the elaboration of theories that are of no use whatever in daily life.<sup>11</sup>

Andrew Pickering's *Constructing Quarks* is a history of particle physics, the structure of which and the understanding of which is determined by a relatively simple and well-defined model of social action. The reference to understanding is important because histories which do not contribute to an understanding of why something happened abound. His characterization of *Constructing Quarks* as a sociological history (as opposed to a historical sociology) emphasizes that the main body of the work is devoted to a narrative of the historical development of particle physics and not to general theorizing about the development of scientific disciplines. Except in the introduction and the last chapter (which serves as a conclusion) — and which together take up less than forty pages of the book's 415 — the sociological commitment is manifested by the manner in which the story is told and in the choice of the events on which the analysis focuses.

The model determines the form of the narrative. It enriches the writing of the history by bringing to light episodes often neglected by traditional historians, episodes which become important from the point of view of the model used. However, Pickering's commitment to a form of 'relativism' also has drawbacks for the writing of his history. He neglects aspects of the dynamics of particle physics that do not readily fit into a model where the 'phenomena' are taken to play a minor role by virtue of their 'flexibility.'

Pickering summarizes his model under the label 'opportunism in context'. The basic notions comprising that model were developed by Barry Barnes in his 1974 book *Scientific Knowledge and Sociological Theory*.<sup>2</sup> Following Barnes, Pickering focuses his attention on the practice of the particle physicists, be they theorists or experimenters. These practices are taken to result from the training of the physicists (which equips them with a set of skills and also with, partly tacit, knowledge) and they define a set of research traditions. In the case of the theorists, the central part of their practice is tied to the use of analogy as a way of developing new theories (in relation to older ones). Pickering argues that 'without analogy, there would have been no new physics' (47). Analogical transpositions are at the heart of theory development. Technical knowledge of some parts of physics and mathematics, and the propensity to use analogy for modelling new theories, are the basic resources of the theoretical physicists. The resources of the experimenters are their apparatus, and their sets of partly tacit technical and technological knowledge.

To understand the dynamics of the particle physics community, Pickering invokes the continual interaction or, as he puts it, the 'symbiosis', of experimental and theoretical practices. The data generated within experimental traditions constitute important resources for the theorists' activities and, conversely, the theories developed by the latter are essential ingredients for developing new experiments and deciding upon the kinds of apparatus to be constructed.

By virtue of their particular training, individual physicists will have different resources at their disposal with which to construct theories or to make experiments. This situation gives rise to different research traditions. These traditions will not be in the same position when facing new experimental data or new theoretical models. For Pickering, the different research strategies developed when confronting these new 'facts' are to be explained in terms of the relative opportunities that different contexts provide for the use of a scientist's particular blend of resources. A consequence of this model of explanation for the historical narrative is that each time a physicist makes a particular contribution to a given problem, his contribution is explained by tracing the previous history of the actor to show that his move was essentially determined by the resources already accumulated and by

the particular context in which the contribution took place. This explains why, in the book, we are often presented with the biography of the main actors.

The acceptance or rejection of any particular experimental data or theory, a process at the centre of the dynamics of the growth of the knowledge in particle physics, is also explained by the notion of 'opportunism in context.' For Pickering, the acceptance or rejection of any particular data or model depends on the ability of one part of the community to use either of them as resources for further research, experimental or theoretical. If the data fit well into the scheme of the theorist, he will not spend much time trying to prove them wrong nor will he ignore them (13). If the data don't fit the theory and the theorist does not see how to modify his model in order to incorporate the data, he will be prone to question the significance of the data; if he can't question the data he may go as far as ignoring them altogether. On the other hand, if he has resources to interpret the 'new data' in a modified model, he will then welcome them as a source for further research. Since the physics community is structured in terms of the resources available to the actors, different camps will usually be formed around different ways to tackle the problems raised by the new data (or the new theory) according to the resources at hand. Moreover, some of the explanations offered by the theorists will open new avenues for experimental work and will be welcomed by the experimental community. This symbiosis of experiment and theory will be self-reinforcing for certain traditions, and self-destructive for those that cannot provide enough resources for the continuation of 'interesting' research programmes. For Pickering, it is this kind of feedback mechanism that explains why certain avenues of research are developed while some others rapidly fade away.

Nowhere in this model is the reality of the phenomena studied by the experimentalists invoked to *explain* the acceptance or rejection of a theory or of new 'facts'. The reason for this is to be found in Pickering's belief that the data generated by experiments do not speak for themselves and can, in principle, be interpreted in a great number of different ways (404). The 'facts' cannot *explain* the choices being made, since they are not unequivocally linked to a single theory. In philosophical *parlance*, Pickering invokes the famous Duhem-Quine thesis (5). Consequently, he also rejects the simplistic analysis which considers theory and experiment to be in adversarial relation, with the latter always deciding the former. For Pickering, theory and experiment are in a relation of 'symbiosis', and the data cannot always have the last 'word' on the theory.

At this point, one might be tempted to discuss the implicit 'relativism' of Pickering's approach. There is, in fact, no reason to take that step for what Pickering is asking is simply not to put the phenomena first, and this point should be taken as a methodological imperative. As Collins has argued,<sup>3</sup> if one is seeking to investigate the *process* of 'construction' of scientific knowledge, one cannot seriously invoke the outcome of that process to *explain* the process itself without going into a circular argument. Moreover, as Collins acknowledges, this requirement is not linked to any kind of relativistic epistemology. Thus, in the study of the dynamics of knowledge 'construction' we should agree with Pickering not to put the phenomena *first*, which, as we will see later, does not mean that we cannot invoke it at the *end* of the process. This difference is important, for we believe that the philosophical conclusions that Pickering tries to draw on the basis of his analysis stem from confusions about the meaning of the Duhem-Quine thesis, and about the significance of the observation that all 'facts' are theory-laden. Before coming to

these somewhat subtler philosophical issues we must first look in more detail at the consequences the use of the model outlined has for the *writing* of the history of science.

### The Flexibility of Phenomena

Pickering's description of the main traditions of theoretical and experimental practices of the HEP community and their transformation over time is convincing and impressive. However, serious problems arise when he attempts to explain the outcome of some of the main experiments and their effects upon theory. So eager is Pickering not to 'put the phenomena first', that he does everything possible to show that the phenomena never limit the possible interpretations that the theory can offer. In so doing, Pickering either forces the argument or neglects some part of his own story, as the following examples will show.

Commenting on the discovery of the neutral current in 1973, Pickering insists that this 'discovery' is inseparable from the new interpretative practices developed by the experimenters in the course of their research. He shows in great detail that the experimenters had to impose an energy cut on the data in order to diminish the background events that could mimic neutral-current events, and he stresses that the crucial step of the discovery reported by the Gargamelle group was to modify a cutting procedure which then became 'standard' in the analysis of the data. The changes were pragmatic and 'potentially questionable' (193). Indeed, 'the Gargamelle discovery was regarded as controversial within the HEP community for several months after its announcement. Only in 1974, following the reported observation of the neutral current by the HPWF collaboration at Fermilab, did the discovery come to be regarded as established' (192). Here it would seem that the 'phenomena' have entered the picture to confirm the discovery of the Gargamelle group, and that an independent observation of the neutral currents played a crucial role in their acceptance by the scientific community. But Pickering argues that this was not the case because 'the HPWF neutral-current was itself grounded in new and potentially questionable interpretative procedures' (19). Thus, it was the new set of procedures that 'made the neutral current *manifest*'. We would agree with Pickering here, although it is not always clear what he wants to conclude from that case, since he also writes that 'these [interpretative] practices brought the neutral current *into being*' (409). Bringing them 'into being' does not exactly mean making them 'manifest'. There is no need to introduce such confusing language to interpret this episode. No doubt the change in the interpretative practices was *necessary* to *make possible* the discovery of neutral current. However, this change was not a *sufficient* reason for a discovery which *before* it was made was only a *possibility*. To equate the change of practice with the existence of phenomena may be opportune from Pickering's point of view, but such a move is based on a *non-sequitur*. Moreover, such an equation suggests that we could produce whatever we want by simply appropriately 'tuning' the apparatus. In short, it is logically incorrect to write that the 'acceptance of the Gargamelle group's novel interpretative practice *implied* at once the existence of the weak neutral current' (405). The only plausible conclusion here is that *if* the experimenters wanted to observe an event like the neutral current they had to calibrate their apparatus in such a way that this kind of

event should not be excluded from observation by the very construction of the apparatus.

Another case in which Pickering's conclusions do not follow from his historical analysis is the episode of the discovery of charmed particles. After having described the events that led to the discovery of the five new particles in 1975 and 1976 that confirmed the predictions of the charmonium model, Pickering notes that 'it is tempting to imagine that the discovery of the five intermediate hidden-charm particles constituted a direct verification of the charmonium model, along the lines implicit in the "scientist's account": the empirical facts proved the theory was right. But if we look ahead to the subsequent fate of the "facts," we will see that such an analysis is untenable' (266). Pickering then goes on to indicate that two of the five particles reported masses and widths that were in conflict with the predictions of the model. Moreover, three years later, 'a highly sophisticated experiment at SPEAR convinced most physicists that the  $\eta_c$  and  $\eta_c'$  did not exist at all — at least not with the masses originally reported. Thus the empirical basis of the charmonium model... was retrospectively destroyed' (267). At this point, one could make the objection that *only two* particles out of five have disappeared, which leaves three particles confirming the theory. But in fact we do not need such a weak argument, for not only is it not the case that the empirical basis was destroyed, the opposite was true: the empirical basis was strengthened. This episode should be rewritten as follows: when the five particles were first reported, two of them did not have *exactly* the predicted properties. At this point, the theorists consoled themselves by saying that more research was needed. The 1979 experiment at SPEAR *resolved* the problem for it found a *new* candidate 'heavier than the previous candidate, at roughly the mass expected from the charmonium model', as Pickering himself explains in a note (276). In this 'revised' version of the episode, the claim that the 'facts' (however constructed and theory-laden) did enter the scene and played a final role in the testing of the theory is indeed tenable, despite Pickering's assertion to the contrary.

Before discussing the more general assumptions behind Pickering's analysis, let us look at a last example of his treatment of experimental data. At the centre of the new physics of the 1970s is the famous Weinberg-Salam model of the weak and electromagnetic interactions. It predicted not only the existence of neutral currents, but also the existence of another important phenomenon — namely, a parity violation in electron-nucleon interaction. The experimenters accordingly set out to find this effect, and in December 1976 two groups working on a similar experiment (one in Oxford and one in Seattle at the University of Washington) reported *preliminary* results (they both indicated that 'there was sufficient interest to justify an interim report') showing that the predictions of the model did not accord with experiments (295). In mid-1978, a Soviet group reported that it had carried out a similar experiment which *agreed* with the predictions of the model. At the same time, the results of a different experiment were reported to be in agreement with the predictions of the theoretical model (SLAC-Yale collaboration) (298). As a result of these developments, a physicist reviewing the situation in 1979 wrote: 'It is difficult to choose between the conflicting experimental results... Tentatively we go along with the positive results of the Novosibirsk and Berkeley groups and hope that *future* developments will justify this step (it cannot be justified *at present* on clear cut experimental grounds)' (300, our emphases).

Again, in this summary the facts seem to enter the scene as a final 'arbiter' in the



discussions. Pickering is well aware of the ensuing danger and writes: 'In retrospect, it is easy to gloss the triumph of the standard model in the idiom of the "scientist's account". But missing from this gloss, as usual, is the element of choice.' For, he argues, 'particle physicists *chose* to interpret them in terms of the standard model (rather than some alternative which might reconcile them with the atomic-physics results) and therefore *chose* to regard the Washington-Oxford experiments as somehow defective in the performance or interpretation' (301, his emphasis). Pickering thus concludes, 'experimental facts did not exert a decisive and implacable influence on theory.' Here we come to the heart of the problems generated by Pickering's approach. Every time data seem to impose themselves on theory, he insists that the physicists always had the *choice* to ignore or to interpret them in another manner. But what does that mean? In the particular case of the Washington-Oxford experiment, no one can doubt that *at that point*, the physicists were bound to make a choice in order to continue their work and the quotation given above makes that plain. The real questions are: What was the basis for the decision taken? Was the choice taken completely arbitrary (as Pickering implicitly suggests) or was it well grounded in the existing sets of data? As the quotation above indicates, the (tentative) choice was simply determined by the fact that there were two *different* and independent experiments agreeing with the predictions and two *similar* ones in disagreement with these predictions. More importantly, there were *other* phenomena which were explained by the theory. This last point is crucial and is always neglected by Pickering. In his analysis of the choices made by physicists, Pickering always gives the impression that the choices are made only on the basis of the data of a given experiment. If that were the case, it would indeed be difficult to justify a choice and it would appear rather arbitrary. In the case of the Gargamelle results, for example, Pickering writes that these 'photographs could have been ascribed a different phenomenological significance from that they historically acquired' (206). In other words, physicists could have devised a different theory to explain the data. Taken in isolation this statement is probably true. The problem however is that meaning is not ascribed to data in isolation, but rather in the light of a whole network relating other explained facts and predicted phenomena. The overall consistency of this network also plays a role in the choice between alternative interpretations or theories.

### Choice and the Possibility of Alternatives

From this point of view the doubts expressed about the Washington-Oxford experiment were well grounded, *even though they could not show that this experiment went wrong*. To state regularly that physicists *could* have made another choice than the one they actually made is not an *argument*, and it does not explain why they have not done so. Neither do such statements imply that the only explanation of their choice lies in the need for social cohesion and opportunity for further research. In fact, we can say, paraphrasing Pickering (242), that given the conflicting data *and* the success of the theory, it would have been almost perverse on the part of the physicist quoted above to have adopted any other position than the one he adopted. More fundamentally, Pickering's frequent statements about the possibility of alternative theories are based on the assumption that it is *easy* to

construct a theory to fit any set of data. Again this may be true for any set of data taken in isolation, but the constraints are more imposing when the theory must fit a great number of different phenomena or data. In other words, we believe that the rigidity of the network is much greater than Pickering allows, and that this rigidity makes it difficult to construct alternative theories (genuinely different and not mathematically equivalent) that can cover successfully a large domain of data related to different phenomena.

The only way to show this in a historical manner would be to look at the fate of alternative theories. In the few cases where Pickering mentions such alternatives to the standard theories of particle physics, he explains their decline in purely social terms: but we believe (though we cannot show it in any detail here) that in all these cases there were always important empirical disagreements and mathematical problems which led to the abandonment of the theory. For example, during the early development of the quark hypothesis, physicists tried for nearly a year to construct a theory which was *simultaneously* invariant under the  $SU(6)$  group and the Lorentz group. They eventually discovered that it was mathematically impossible to do so (non-existence theorem) (94). We could similarly cite the case of the models developed *before* the discovery of neutral currents that tried to modify the simpler Weinberg-Salam model in order to suppress the occurrence of neutral currents (which were then unobserved). Naturally, they were abandoned after the discovery. A detailed account, which would be necessarily technical, would show that physicists had trouble finding a suitable model which would account for the non-existence of neutral currents and at the same time still fit the existing data on scattering phenomena. Of course, the collective behaviour of the physics community plays a role in the evaluation of these technical difficulties, and this evaluation determines the extent to which alternatives will be pursued. But this does not exclude the fact that *technical* problems (as opposed to social consensus) can be a *sufficient* reason to abandon a particular theory.

### Facts, Theories, Apparatus and the Duhem-Quine Thesis

It should be noted that our point of view is not equivalent to the 'scientist's account' so disdained by Pickering, nor does it imply that we are 'putting the phenomena first'. It is only an acknowledgement of the fact that technical problems, existing data (or non-existent data) and mathematical problems *also* play a role (and an important one) in determining the outcome of the relations between theory and experiment, as the examples discussed above clearly show. At this point, Pickering would surely invoke what he considers a 'forceful philosophical objection' (5) against the simple view of theory testing — namely, the famous Duhem-Quine thesis about the underdetermination of a theory by facts, and its implied wholeness, which precludes the localization of the false hypothesis (or law) in the theoretical structure of the theory in the case of disagreement with experiment. Curiously enough, this philosophical thesis is often quoted as a 'fact' by sociologists who use it as a 'weapon' against rationalists. However, the problem is not so simple, for the implied wholeness is predicated upon a hypothetico-deductive model of scientific theory which can itself be shown to be inadequate. In fact Clark Glymour has recently suggested a model of scientific theory in which the Duhem-Quine thesis is considerably weakened.<sup>4</sup> Glymour's 'bootstrap' model of theory testing makes

possible the localization of a false hypothesis, and hence the local testing of a theory. By the way, it also suggests a simple rational explanation for the fact that scientists *do* localize problems in theories, a move which from the point of view of the Duhem–Quine thesis can only be completely arbitrary and conditioned by causes external to the relation between the theory and the data. Of course, this is exactly the kind of explanation offered by Pickering. Though this is not the place to discuss Glymour's model, we have referred to it as an antidote to the simplistic use of complex philosophical problems, and in order to suggest that the Duhem–Quine thesis does not necessarily cut that much ice.

This brings us to a different but related philosophical problem discussed by Pickering — namely, the oft-quoted 'theory-ladenness' of facts. According to Pickering, 'if explanatory theories are implicated in the construction of empirical facts and phenomena, then theory cannot be absolutely constrained by experiment' (406). To say that theories are implicated in the production of empirical facts does not necessarily entail that a theory cannot be absolutely constrained by experiments. To make the needed implication, as Pickering does, one must suppose that it is the *same* theory which is at work in the experiment and which is being tested. However, this is usually not the case, as the following examples will show.

A classical test of quantum electrodynamics (QED) is the famous Lamb shift. The measurement of the Lamb shift is based on concepts pertaining to spectroscopy and 'classical' quantum mechanics, and is not based on QED. In point of fact, a reliable, usable QED did not exist at the time the experiment was made. The same could be said about the experiment that led to the discovery of the J-Psi particles. It was the result of 'serendipity', for the peak appeared during a general search for vector mesons produced in electron-positron annihilation (258). Hence these experiments could test any theory and no circularity would be involved. So, contrary to Pickering's statement, the new physics of the 1970s is no more and no less 'theory-loaded' than the old physics, and it is ambiguous to say that 'the new physics was theory-loaded' (353), or that 'high resolution detectors had the prejudices of the new physics built in' (358), for it confuses different levels of analysis. Of course, the instruments of the new physics were constructed so as to *exclude* a lot of the common phenomena of the old physics in order to make visible the rare phenomena predicted by the new physics and, as a consequence, 'they were impotent to cope with the high event-rates associated with the more common process' (364). In this sense they were indeed 'prejudiced' — but no more so than in any other situation where apparatus is used to test the prediction of a theory. Empirically testing a physical theory implies the construction of a *specific* apparatus which by definition excludes the observation of other phenomena. Thus it should not come as a surprise that the detectors constructed for the new physics will not yield low-energy data, any more than a voltmeter measures an electric current. The fact that an apparatus is constructed for a given experiment, and that it can be more or less flexible, has nothing to do with the fact that apparatus are theory-laden. Indeed, the observation that apparatus is in a sense the incarnation of theories was pointed out long ago by Gaston Bachelard, and this never led him to relativism.<sup>5</sup> Nor does it imply that theories cannot be tested because, as we have said, the theory of the apparatus is rarely the same as the theory to be tested by the apparatus. To conclude, as Pickering does, that because the new instruments precluded the visibility of low-energy phenomena they define a new phenomenal

world, and that each of these worlds is a 'self-contained, self-referential package of theoretical and experimental practice' (411) is, to say the least, an exaggeration.

As we have noted, there is no conceptual self-reference involved in the cases discussed and, though the theory can suggest the construction of a particular apparatus (as was also the case in classical physics), this does not mean that the instrument will necessarily yield data consistent with the theory. In a trivial sense, though, there is a 'social self-reference' involved here, for the decision to construct a high-energy accelerator and to close a low-energy accelerator is a social decision, one that can cost the jobs of many low-energy physicists and may even stop the production of knowledge in that domain. In this sense, the new physics can exclude the old physics.

In short, the self-referential nature of theory and experiment suggested by Pickering — reminiscent of the Leibnizian monads that cannot communicate with another — does not follow from his case studies. In addition, the 'global incommensurability' that Pickering sees between the old physics and the new physics is superficial, for it boils down to an assertion that, for example, rheologists live in a 'different world' than, say spectroscopists because they don't use the same instruments and the same theories, and they can speak to each other only with great difficulty. No one will deny that. Hence, to write that 'the interlinked differences between the natural phenomena, explanatory theories and experimental strategies of the old and the new physics... constitute the grounds for a simple and direct diagnosis of incommensurability' (411) is too simple. At best Pickering has constructed a good metaphor. But even that needs qualification, for if it is true that 'the old physics has nothing to say on the rare phenomena of the new physics', it is not true that the 'new physics theories had nothing to say on the common phenomena of the old physics' (411).

### Conclusion

In conclusion, the bulk of *Constructing Quarks* can be used as an outstanding history of HEP. Pickering's book clarifies the concepts involved by giving their history. He rightly insists on the role of analogies in theory construction, and on the importance of looking at the theoretical and technical resources available to the community, as well as to the context in which these resources are used, in order to understand why a particular solution to a problem was adopted. However, as we have suggested in this Review, this approach does not depend on any commitment to a programme of 'relativism' which, in the few instances where Pickering has tried to use it, has led him to a misleading analysis of the historical situation.

### • NOTES

1. Warren Hagstrom, *The Scientific Community* (New York: Basic Books, 1965), 9.
2. Barry Barnes, *Scientific Knowledge and Sociological Theory* (London: Routledge & Kegan Paul, 1974).

3. See, for example, H. M. Collins, 'What is TRASP?: The Radical Programme as a Methodological Imperative', *Philosophy of the Social Sciences*, Vol. 11 (1981), 215-24.

4. Clark Glymour, *Theory and Evidence* (Princeton, NJ: Princeton University Press, 1980).

5. See G. Bachelard, *Le Rationalisme Applique* (Paris: Presses Universitaires de France, 1949). For an introduction to his thoughts, see Mary Tiles, *Bachelard: Science and Objectivity* (Cambridge: Cambridge University Press, 1984).

**Yves Gingras** is a Social Sciences and Humanities Research Council of Canada Post-Doctoral Fellow at the Department of History of Science, Harvard University. He is currently working on the sociology of specialty formation in twentieth-century physics, and has also published articles on the history of physics in Canada.

**Silvan S. Schweber** teaches Physics and the History of Western Thought at Brandeis University. He is presently completing a book on the history of quantum field theory and the theoretical physics community in the period from 1938 to 1952. *Authors' addresses* (respectively): Department of Sociology, University of Montreal, Case Postale 6128, Suc. A, Montreal, Quebec, Canada H3C 3J7; Department of Physics, Brandeis University, Waltham, Massachusetts 02254, USA.